

YOU CAN LEAD AN APE TO A TOOL, BUT . . . : A REVIEW OF POVINELLI'S FOLK PHYSICS FOR APES: THE CHIMPANZEE'S THEORY OF HOW THE WORLD WORKS

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We reviewed Daniel Povinelli's book, *Folk physics for apes: The chimpanzee's theory of how the world works*. After a summary of the book's contents, we analyzed two sets of experiments on chimpanzees' folk psychology: one that explored whether chimpanzees understand that others see (i.e., that apes have internal visual experiences) and another that examined whether chimpanzees can distinguish intended from unintended actions. The conceptual scaffolding on which these studies were conceived was sufficiently faulty that their outcomes were virtually assured a priori. We then analyzed two sets of experiments on chimpanzees' folk physics, reinforcing our view that conceptual confusion guaranteed that certain key predictions about the outcome of these studies could not be supported. A unifying reason for this conceptual confusion is that the author devalues *understanding* that results from programmatic conditioning. We closed the review by relating Povinelli's findings and conclusions to behavior analysis and by explaining why behavior analysts should read this book

Key Words: conceptual analysis, tools, understanding, folk physics, folk psychology, chimpanzee

We do not say that *possibly* a dog talks to itself. Is that because we are so minutely acquainted with its soul? Well, one might say this: If one sees the behavior of a living thing, one sees its soul. (Wittgenstein, 1958a, p. 113e in *Philosophical Investigations*)

The mental features discoursed of as the analytical, are, in themselves, but little susceptible of analysis. We appreciate them only in their effects. (Edgar Allan Poe, 1841/1992, p. 141 in *The Murders in the Rue Morgue*)

We are all familiar with the intuitive concepts of force, weight, speed, and shape, and use them in our dealings with objects and people. These concepts are often interconnected, such as when we say that we need to push harder (a matter of force) to move heavier objects (a matter of weight), or that broad objects fall slower to the ground than narrow objects. The preceding statements express a set of common sense beliefs that may be collectively referred to by the picturesque term "folk physics." But when these beliefs are extended to chimpanzees by entitling a book *Folk Physics for Apes*, then we are twice puzzled. First, in what sense can any nonverbal creature, whether a cockroach or a chimpanzee,

hold a similar set of beliefs? And second, what should we make of the preposition *for* in the book's title? Will someone be teaching folk physics to apes (cf. *Statistics for Social Scientists*), or does the title refer to something (folk physics) that is used by someone (apes), similar to *Dosing Tables for Physicians*? Our puzzlement is then compounded by the book's subtitle, *The Chimpanzee's Theory of How the World Works*. Regardless of how stringently or loosely the word "theory" is used—compare, for example, the physicist's theory of universal gravitation, the biologist's theory of evolution by natural selection, the psychologist's theory of attachment, and the detective's theory of the crime—in every case the word points to a set of interconnected statements describing, summarizing, integrating, or explaining a set of facts or occurrences. It is, thus, reasonable to expect theories of how the world works only from creatures capable of making and revising statements, asking and invoking reasons, entertaining or conjecturing ideas. Cockroaches, birds, cats, dogs, monkeys, apes, and even human infants do not build, entertain, or test theories of the world. With a mysterious title and a provocative subtitle, the author has succeeded in capturing our attention.

We begin our review with a summary of the book's contents. Next, we critique Povinelli's work on chimpanzees' folk psychology by focusing on two sets of experiments: one that

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explored whether chimpanzees understand that others see (i.e., that apes or people have internal visual experiences), and the other that examined whether chimpanzees can distinguish intended from unintended actions. We suggest that the conceptual scaffolding on which the studies were conceived was sufficiently faulty that their outcomes were virtually assured a priori. In the third section, we analyze two sets of experiments on chimpanzees' folk physics, reinforcing our view that conceptual confusion guaranteed that certain key predictions about the outcome of these studies could not be supported. In the fourth section, we discuss a unifying reason for this conceptual confusion: the devaluation of *understanding* that results from programmatic conditioning; and one of its unfortunate but predictable consequences: relatively uninformative conclusions. In the fifth and sixth sections we relate Povinelli's findings and conclusions to behavior analysis, focussing on the idea that causality is in the interaction. We close the review with an explanation of why *Folk Physics for Apes* should be read by behavior analysts. Let us open the book and see what is inside.

GENERAL CONTENT OF *FOLK PHYSICS FOR APES*

There has never been a shortage of interest in humans' closest genetic relative, the chimpanzee. Countless articles, books, movies, and television programs have been produced for the serious scholar, amateur naturalist, and the curious layman. The goals of most of these works are comparative, and usually attempt to show how much of human physiology and behavior is present in chimpanzees or, less often, how much of chimpanzees' physiology and behavior is present in humans. For example, a recent well-publicized program on the Discovery Channel, *Keeli and Ivy: Chimps Like Us*, documented Sally Boysen's attempts to teach chimpanzees to read, write, and produce symbol or letter combinations similar to that used in human language.

In *Folk Physics for Apes*, the well-known expert in cognitive evolution from the University of Louisiana, Daniel Povinelli, seeks the same general comparative goal of looking for human abilities in the chimpanzee. The au-

thor describes 27 previously unpublished experiments that he and his collaborators conducted over 5 years to explain the nature of chimpanzees' understanding of the causal structure of the world surrounding them; in particular, the causal structure involved in the use, construction, and modification of tools. In the wild, this might consist of an adult chimpanzee placing a nut on top of a stone, grasping another stone, and then using it as a hammer to crack open the nut; or of a chimpanzee searching for and finding a twig, using its fingers to remove the leaves attached to the twig, and then inserting the twig into a nest of termites to catch the insects. Povinelli and his collaborators set out "to explore how chimpanzees conceive of the physics that underlies their use and construction of simple tools. . . to elucidate the nature of the mental representations that guide this behavior. In short, we want to explore their 'folk physics' of tool use and manufacture" (p. 1). Povinelli is interested in a kind of understanding that "develops naturally and spontaneously" (p. 2) in the course of chimpanzee development, rather than an understanding that is created through explicit training by experimenters because, after all, folk physics is a *naturally-occurring* knowledge of the physical world.

The book is divided into 12 chapters. The first three set the historical, conceptual, and methodological stages where the experiments described in the next eight chapters took place. The last chapter summarizes the main experimental findings and discusses their implications, ultimately linking the constellation of results to a scenario about the evolution of a kinesthetic self-concept required for arboreal clambering. The bulk of the book is therefore composed of the eight chapters that describe the problems posed to the 7 chimpanzees used throughout the studies reported in the book, the techniques and methods of data analysis, the obtained results, and their implications. Each of these chapters is written as an abbreviated research paper on a specific type of tool-use problem. The writing is simple and clear, the pictures and illustrations superb, the data analyses adequate, and the results interesting. The author is at his best when conceiving his ingenious experiments and analyzing their findings. He is at his worst when conceptualizing the re-

search problems and drawing broader implications from the results.

The experiments are organized around different conceptual groups: understanding of gravity, transfer of force, size-shape interactions, physical connection, and object transformation. A common procedural variable across all 27 experiments was that the apes had to use a tool to retrieve a reward (e.g., a piece of an apple). Before the experiment began, the tools were introduced into the research area to familiarize the apes with them. If the animals did not interact with the tools spontaneously, they were encouraged by the trainers to do so. After some experience with the tools, the experiment proper began with a brief amount of training on how to use the tool to solve the experimental problem, such as inserting a stick into a transparent tube to obtain a reward inside it; pulling a rake, rope, or piece of cloth to retrieve a reward out of arm's reach; straightening a pliant tube and inserting it into a narrow opening to obtain a reward; and the like. In some cases the training consisted exclusively of actual manipulations of the tool by the ape, but in other cases the experimenters first demonstrated how to use the tool. A key point is that the amount of training was deliberately kept to a minimum because the author was interested in what the apes "understood ahead of time, not what they could learn through trial-and-error" (p. 35). Finally, a set of transfer tests attempted to unravel what the chimpanzees knew about solving the problem.

The transfer tests pitted two or more models against one another. At one end of the model continuum was what the author called a sparse or "low-level model," a model according to which the chimpanzees learn only a "procedural rule, with no accompanying appreciation of one or more of the causal relations involved" (p. 116). At the other end of the continuum was a "high-level model" whose key assumption was that the apes understood the critical causal relations involved in the test. Although the author did not state it this way, one could say that the low-level model made no inferences about mental representations and instead accounted for the chimpanzees' behavior in terms of basic learning principles (e.g., stimulus discrimination and generalization, overshadowing, operant conditioning, and Pavlovian condi-

tioning). In addition, given the quality and quantity of training, the low-level model also predicted that the apes' behavior would be highly context specific. In contrast, the high-level model inferred complex cognitive processes and accounted for the animals' behavior in terms of abstract relations easily transferred to novel contexts.

CHIMPANZEES' FOLK PSYCHOLOGY?

To begin, we need to emphasize the fact that, although there are profound psychological differences between humans and chimpanzees, there can be absolutely no doubt that chimpanzees are alert, thinking organisms, who are finely attuned to the complexities of the social and physical universe that unfolds around them. Simply because their minds differ from ours in some rather profound ways, does not imply that chimpanzees are 'black boxes' (as behaviorists would have it), devoid of internal representations. Quite the contrary. Chimpanzees, like most animals, should be regarded as cognitive creatures—organisms whose senses receive information about the world and whose brains translate that information into a neural code that reduces and 'represents' the external world in a way that can later be used to support their behavior. Thus, any discussion of the 'psychology' of chimpanzees must begin with an unwavering affirmation that they are cognitive creatures. (p. 58)¹

Before delving into folk physics, Povinelli and his colleague, philosopher Steve Giambrone, devote considerable space to a review of chimpanzees' folk psychology (i.e., chimpanzees' naive, common sense understanding of other chimpanzees' and humans' minds). As Povinelli puts it, "we examine whether chimpanzees, like humans, *appreciate* [italics added] that other organisms have internal psychological experiences such as desires, emotions, beliefs, and plans. When chimpanzees look at, and interact with each other, do they realize that they are dealing

¹ At several places in the book there are unnecessary and anachronistic (if ever accurate) views of behaviorism. We are unaware of any major behaviorist's writings in the past 50 or more years claiming that animals are "black boxes" or that we should not study thoughts, feelings, sensation, ideas, and the like. For reasons concerning this and other distortions, see Machado, Lourenço, and Silva (2000).

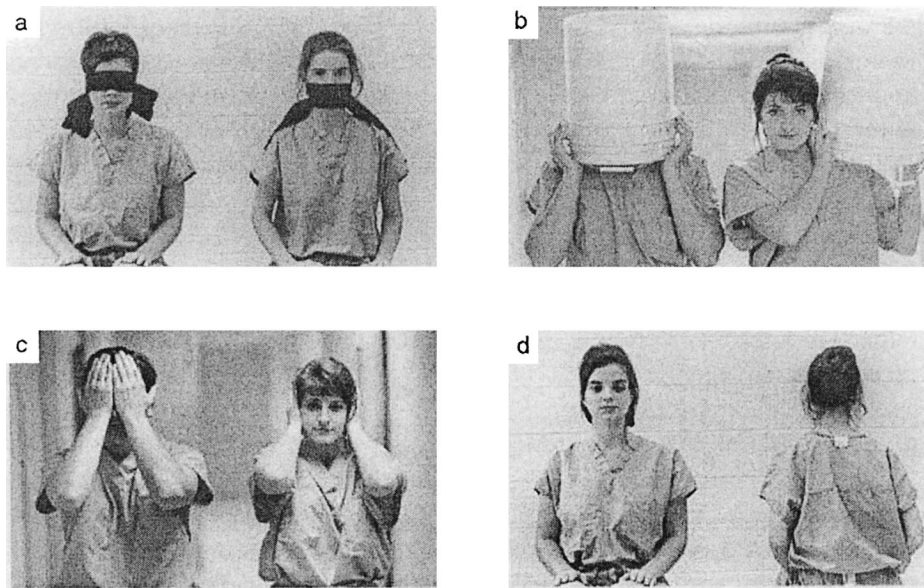


Fig. 1. Conditions used in one of the seeing/not-seeing experiments. Panel a depicts the blindfold test; Panel b, the buckets test; Panel c, the hands-over-the-eyes test; and Panel d, the back/front test. From Povinelli, D. J. (2000). *Folk physics for apes: The chimpanzee's theory of how the world works*. Oxford, England: Oxford University Press. Reprinted with permission.

with both an observable, physical body and an unobservable, subjective mind?" (p. 7). In other words, do chimpanzees possess second-order mental states (i.e., mental states about the mental states of other apes or humans)? These broad questions of folk psychology are first narrowed down to questions concerning *seeing*, that is, questions of whether chimpanzees "*appreciate* [italics added] that others have internal, visual experiences. In short, whether they know that others 'see'" (p. 19).²

To answer these and similar questions Povinelli constructed a series of ingenious experiments. For example, in one set of experiments the apes had a choice between begging for food from an experimenter who could see them or from an experimenter who could not (the apes had already learned to extend their arms to beg for food from their trainers). In one condition, an experimenter had her eyes blindfolded and another experimenter had a similar blindfold covering her mouth (Figure 1, Panel a). Would the apes beg preferentially from the person who could see them? Did they understand that they had to be seen for their begging to be effective?

² We note the frequent use in the book of the vague and noncommittal term *appreciate*.

In all conditions except the back/front one (Figure 1, panel d) the apes did not prefer to beg from the experimenter who could see them. That is, they were indifferent or, in the authors' words, "oblivious to the psychological distinction between the two experimenters" (p. 32). Given the disparity of the results across conditions, Povinelli and his collaborators then embarked on an elaborate, detective-like search to discover which stimulus features controlled the response of begging. The additional experimental conditions show Povinelli at his best: He had experimenters looking over their shoulders, covering their faces with screens, distracted and looking away from the apes, wearing masks with two holes for the eyes, and so on. In the end, the pattern of results suggested the following hierarchical rules: (a) gesture to the person whose front is facing forward; (b) if both fronts are present (or absent), gesture to the person whose face is visible; and (c) if both faces are visible (or occluded), gesture to the person whose eyes are visible. "*Seeing*, then, did not appear to be a concept recruited by the chimpanzees to help them decide to whom they should gesture" (p. 38).

Other experiments on chimpanzees' folk

psychology explored gaze following, referential pointing, attention-getting behavior, cooperation, and distinguishing intended from unintended actions. In every case the experimental outcome was the same: "Models which posit that chimpanzees reason about behavioral propensities, but not mental states, have consistently done a better job of predicting how chimpanzees will behave in crucial experimental situations. At the same time, these models provide an account of how their natural, spontaneous social behaviors can be generated without appealing to second-order mental states" (p. 72).

But to reach these conclusions seemed to have been a painful process for the author: "*To our surprise* [italics added] . . . the apes did not prefer to gesture to the person who could see them These results made a *deep impression* [italics added] on us. . . . *More disturbing still* [italics added], the results seemed to imply that even for the back/front condition our apes might have no idea that the experimenter facing away was 'incorrect' *We had difficulty accepting the implications of these results* [italics added]" (p. 34). As we argue below, however, the author's discomfort could have been avoided had he realized either the surface improbability of the predictions of the high-level model or the incoherence of its fundamental assumptions.

Seeing and Seeing Intentions

Given the choice of begging for food from a forward-facing experimenter who is covering her eyes with her hands or from a forward-facing experimenter who is covering her ears with her hands, what will a chimpanzee do? If the chimpanzee understands that it can only receive food from someone who sees it, then it will beg for food from the person who has covered her ears; if the ape lacks such understanding, then it will beg equally from both experimenters (which is precisely what the chimpanzees did). Inspired by a high-level, folk-psychology model, this seemingly straightforward experiment has two conceptual shortcomings. First, the model leaves completely unexplained why, in the absence of explicit training, a chimpanzee would beg more from a person looking at it than from someone not looking at it; or why it would know that the eyes-looking-at-it, but not the experimenter's nose, mouth, chin, or hand

with the food, was the critical feature. Without knowledge of the intercorrelations among the stimulus features *nose, hand, eyes*, and the like, on the one hand, and the correlation between these features and the act of giving food for begging, on the other hand, the predictions of the high-level model remain incomprehensible.³

Second, and more serious, the high-level model's root assumption that the mind of an animal is an internal causal process leads to deep misconceptions about seeing, in particular, and the intentional idiom, in general. Consider how Povinelli contrasts the low- and high-level models of seeing:

But, here again, the ugly problem arises as to whether these behaviors indicate that the apes are reasoning strictly about your behavior ("Hey, hurry up and give me some juice!") [the low-level model] or about both your behavior and your internal visual/attentional state ("Hey, look at me! Now, hurry up and give me some juice!") [the high-level model]. Although this distinction may seem minor, it is exactly the difference between an organism who understands others as physical bodies and an organism who appreciates that there are minds within those bodies. (p. 55)

That is, the command "Hey, look at me!" is assimilated to reasoning about an internal mental state. To see the error of this assimilation, we invite the reader to consider how one learns this and similar commands, in what contexts they are uttered, and for what purposes. More generally how does one *use* them? Regardless of the specific details, the reader's answers will point to the contexts in which an action (verbal or otherwise) directed toward another person becomes more effective when preceded by the command "Hey, look at me!" than when it is not. Hence, the distinction alluded to by Povinelli is not between physical bodies and minds *within* those bodies, but between physical bodies and responsive organisms.

³ Consider a chimpanzee that is rewarded for pressing a green key with a black vertical line. Will it understand or know which stimulus, the green background or the vertical line, was the target feature? "Of course not," some might say, "but eyes are different; they are signs of attending, clear windows to the mind of another ape." But ears, arms, or other body parts may also be signs of attending. And as for windows to the mind, none is clearer than the very actions of the ape.

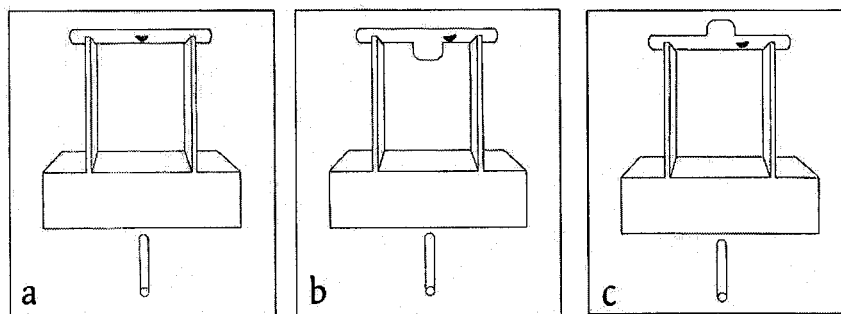


Fig. 2. Apparatus for one of the tube-trap experiments. Panel a shows a training configuration, Panel b shows a testing configuration, and Panel c shows a testing configuration when the trap was inverted. From Povinelli, D. J. (2000). *Folk physics for apes: The chimpanzee's theory of how the world works*. Oxford, England: Oxford University Press. Adapted with permission.

The same conceptual shortcomings occur in other studies, such as when the author examined whether chimpanzees can discriminate intended from unintended actions. To answer the question, a chimpanzee watches one person accidentally spill a cup with juice and another person deliberately pour the juice onto the floor. Afterwards, when given a chance to beg for juice from these two people, from whom will the chimpanzee beg? According to the high-level, folk-psychology model, if the chimpanzee understands the difference between intentional and unintentional actions, then it should beg from the person who accidentally spilled the juice. However, in the absence of a particular learning history, strongly shaped by language, how and why would a chimpanzee understand the difference between these actions? On this point, the high-level model is embarrassingly silent.

To illustrate further, consider the following classroom exercise inspired by Wittgenstein (1958b). Ask a student to raise her arms voluntarily and she will readily do so. Next, ask her to raise her arms while you forcefully push them down. After trying, she will reply, "I can't. You're holding them down." Finally, ask the student to raise her arms involuntarily. Perhaps smiling to hide a strange embarrassment, she will typically say again, "I can't." But what is it that the student cannot do in the latter case? (Compare the two uses of "I can't.") One point of the exercise is to show that there is nothing in the *topography* of an intended or voluntary action that distinguishes it from an unintended or involuntary one. How, then, could an ape know

the distinction on the basis of the experimenter's actions alone?

Having misconceived of the mind as an internal entity, the high-level model was forced to compound the error by misconstruing the idiom of *seeing* as talk about internal states (see Machado, 1999) and the idiom of *intention* as talk about the topography of behavior. The failure of the high-level model to predict the chimpanzees' behavior was inevitable and therefore any conclusions derived from the model's failure are relatively uninformative.

CHIMPANZEES' FOLK PHYSICS?

The wealth of research on tool use and tool construction by chimpanzees and other non-human primates has not addressed the fundamental distinction between understanding *that* tools work versus understanding *why* they work. (p. 3)

In the preceding section, we identified some examples of conceptual confusion inherent in the high-level model; confusion that made outcomes of the experiments on chimpanzees' folk psychology unlikely to support the model's predictions. In the present section, we analyze examples of similar conceptual confusion in experiments on chimpanzees' folk physics.

Trap-Tube Problem

Five apes initially learned to use a stick to retrieve a piece of fruit or cookie from a horizontal, clear tube (Figure 2, Panel a). Then the tube was replaced by a similar tube with a vertical trap (Figure 2, Panel b). Surprisingly, perhaps, after 140 trials only one ape, Me-

gan, had learned to avoid the trap by inserting the tool into the opening farthest from the reward. The remaining animals either always inserted the tool on the same side (a position bias) and therefore secured the reward on only half of the trials, or always inserted the tool into the closest opening to the reward and therefore never obtained it.

But what had Megan learned? According to Povinelli:

... at least two distinct possibilities exist. On the one end, she may have learned a procedure based upon the distance of the reward from the openings of the tube, which could be described verbally as "insert the tool into the opening farthest from the reward." On the other hand, as an alternative (or in addition) to this rule, Megan may have learned something about the causal structure of the task. In particular, she may have represented the connection among the three central elements of the task: (1) her actions on the tool; (2) the tool's action on the reward; and (3) the trap's action on the reward. Indeed, it is the last relation in which we are most interested here, because it was this relation that was crucial to solving the transfer test. (pp. 115–116)

The two models differ considerably in precision. The low-level model identifies a clear feature of the stimulus that may control the ape's action of inserting the tool into the tube. In contrast, the high-level model states that the animal may have learned or represented *something* about how the elements of the task are interconnected. Besides the vague "causal structure of the task," what that something may be is unspecified.

To probe what Megan might have learned in the trap-down condition of Figure 2, the author rotated the tube 180° so that the trap, now pointing upwards, became irrelevant (Panel c in Figure 2). According to Povinelli, the two models were directly pitted against each other:

If the low-level model were correct, and Megan was using a rule to insert the tool into the opening farthest from the reward, without considering how the trap functioned to capture the reward, she should be expected to continue to use this procedure *even when the trap was inverted*. Alternatively, if the high-level model were correct, Megan could be expected to either insert the tool at random into either opening in the tube, or always insert it into

one opening, because with the trap inverted it made no difference where the tool was inserted. (p. 116)

In 39 of 40 trials, Megan used the procedural rule and behaved as if the trap were still in effect. Moreover, even when Povinelli biased the location of the tool by placing it next to the opening closer to the reward instead of along the tube's midline, Megan still inserted the tool into the opening farthest from the reward. These results strongly supported the low-level model.

In a subsequent experimental condition, the author further biased the location of the tool by inserting it slightly into one or the other openings of the tube. When the tool was already partly inserted, Megan simply pushed it. Needless to say, when the trap was down and the tool was partly inserted into the opening closest to the reward, Megan never obtained the reward. These results suggest that Megan had acquired a response chain with the following behavioral links (i.e., critical stimulus → response): (a) tool outside the tube → grasp the tool, (b) location of reward with respect to trap → insert the tool into the opening farthest from the reward, and (c) tool inside the tube → push the tool until the reward is obtained. Hence, when Megan entered the room and saw the tool already partly inserted into the tube, the chain started at (c) instead of (a), and she simply pushed the tool. In any case, her behavior never covaried with the up or down orientation of the trap.

Four years later, when the apes averaged 9 years of age, Povinelli repeated the experiment and obtained the same results with Megan—the ape remembered all of it! In addition, two other apes, Candy and Brandy, eventually learned to solve the trap-down problem through shaping and modeling. In transfer tests, they behaved like Megan in all ways except one: Brandy inserted the tool into the opening farthest from the reward even when the tool was already partly inserted into the tube.⁴ In no case was there any evi-

⁴ The difference between Brandy's results and Candy and Megan's may be interpreted in terms of differences in the *response units* composing the chain. For Brandy, the chain might have been: (a) location of reward with respect to trap → grasp tool and insert it into the opening farthest from the reward, and (b) tool inside the tube → push the tool until the reward is obtained.

dence that the orientation of the trap controlled the animals' behavior. Povinelli interprets these results as follows:

[T]hese animals were not *conceptualizing* [italics added] the trap's up or down configuration as being relevant to their actions with the tool

We can summarize what the successful apes learned during the original trap-tube task as follows. They began by preferring to insert the tool consistently on one side of the tube, thus losing the reward on exactly half of the trials. Gradually, they began to vary the side into which they inserted the tool, thereby *learning a rule* [italics added] to insert the tool in the tube opening farthest from the reward. In the case of Megan and Candy, once this behavior had been reinforced, *the rule appeared to become routinized* [italics added] as a series of discrete, non-reversible steps. However, it is possible that these animals' apparent lack of understanding could be related more centrally to an *inability to inhibit a learned sequence of actions* [italics added]. Thus, Megan and Candy may have been able to *mentally represent* [italics added] the result that their actions would have, but were *unable to inhibit* [italics added] carrying the tool to the side consistent with the rule, or, in the tool-in-tube conditions, pushing the already inserted tool. If true, this inhibitory problem would set limits on their ability to exploit other possibilities for success.

Brandy differed from Megan and Candy only in that she was able to *implement the core procedural rule* [italics added] ('insert the tool into the opening farthest from reward') even on those trials where we had already inserted the tool into one of the openings. Brandy's results also cast doubt on attributing Megan and Candy's behavior strictly to inhibitory problems. After all, Brandy was at least somewhat capable of inhibiting the *prepotent action* [italics added] of pushing the tool in the tool-in-tube condition, yet her *underlying rule structure* [italics added] nonetheless appeared to be unrelated to the orientation of the trap. (p. 130)

This interpretation is unnecessarily convoluted. First, it makes no sense to say that nonverbal creatures conceptualize a stimulus as being relevant or irrelevant to their action. Animals may learn to behave in one way in the presence of a class of stimuli—a class defined, for example, by a set of features common to its elements—and in another way in the presence of another class. And psychologists may designate this type of learning as

concept learning without thereby introducing confusion into their accounts. But to conceptualize means more than to learn a concept. Among other things, it means reasoning with concepts, connecting them in particular ways, and drawing inferences from their use. How this is reasonable (let alone true or false) in nonverbal creatures remains to be explicated. Moreover, to say that the apes did not conceptualize the trap's configuration as being relevant to their actions—as opposed to simply saying that the orientation of the trap did not influence the apes' behavior—turns a straightforward behavioral observation that is ready to be analyzed empirically into a mysterious cognitive process. What is gained in mystical awe is lost in scientific understanding.⁵

Second, it is misleading to say that the apes learned a rule when the only thing we have observed, and the only thing we can say intelligibly of nonverbal creatures, is that their behavior accords to a rule. This is not verbal minutia, for it has important implications. One of them is that instead of saying that the rule learned by the apes became routinized—whatever that actually means—we would say that their *behavior* became routinized, that it is context-insensitive or automatic, such as when the habit of entering the kitchen and turning to the right to reach the refrigerator persists in a new house because it was appropriate in the old one. Another implication is that instead of conceiving of the difference between Brandy's behavior and Megan and Candy's in terms of differences in underlying rule structure—again, whatever that actually means—we conceive of this difference in terms of overt *behavioral* structure (e.g., the units of their respective response chains). The general point is that to promote behavioral regularities to the status of rule-governed behavior is another expres-

⁵ It cannot fail to impress the newcomer to psychology that the discipline is dominated by a curious bias from the standpoint of scientific research. We refer to psychologists' predilection toward the mysterious at the cost of the observable and measurable. Given two hypothetical accounts of a behavioral pattern, one appealing to variables x_1, x_2, \dots, x_n , each well defined conceptually, with clear dimensions, and relatively easy to measure, and another appealing to variables y_1, y_2, \dots, y_n , each poorly defined conceptually, without clear dimensions, and not easily measurable, the psychologist will curiously and consistently prefer the latter.

sion of the unfortunate tendency to replace straightforward behavioral facts, ready to be analyzed empirically, by mysterious cognitive processes. What is lost by this tendency is clear; what is gained is not.

Third, it is not parsimonious to hypothesize mental representations, central inhibition, and prepotent actions to account for the apes' behavior. Reconsider the basic findings: (a) the apes' behavior of inserting the tool was not controlled by the orientation of the trap, and (b) Megan and Candy but not Brandy pushed the tool when they found it already inserted into one of the openings. Having assumed that the animals might have mentally represented the result of their actions (let us call this Assumption 1), Povinelli is forced to add the idea that Megan and Candy could not inhibit (centrally, we are told) their action of carrying the tool to the particular side consistent with the rule (Assumption 2) except when the action was prepotent (Assumption 3). Brandy, however, "was better able to execute the general procedural rule" (p. 129) or "to reverse the order of steps in her learned actions" (p. 130) (Assumptions 4a and 4b). The concepts of prepotent action, central inhibition, and mental representations make the account not only excessively ad hoc but also more obscure than the facts themselves.

From a behavioral perspective, the finding that the orientation of the trap did not influence the apes' behavior is not particularly surprising because the animals learned to retrieve the reward with the trap always down. Lack of variation in the trap does not favor the behavior of attending to it.⁶ Furthermore, the behavior learned during the trap-down condition remained appropriate during the trap-up condition, so there were few or no reasons for the apes to alter their behavior. We believe that to promote stimulus control by the orientation of the trap, the following elements of the learning situation should have been varied early in training: the position of the trap (i.e., up and down, left and right), the apes' actions of inserting the tool

into the two openings, and the consequences of those actions. As for the differences between Brandy and the other two apes, we have interpreted them above in terms of differences in response units. Although we do not know why these differences occurred, it seems better to recognize ignorance than to hide it under a multitude of ad hoc and elusive concepts.

After interpreting the experimental findings, Povinelli concludes:

In general, the results . . . favor the hypothesis that our chimpanzees did not understand how the trap functioned in the context of the causal interactions among the tool, the reward, and the trap itself. Clearly there are any number of reasons why this may be true. One reason. . . is that chimpanzees do not invoke a priori theoretical concepts (such as gravity) to mediate their use of tools. (p. 131)

The author's conclusions evoke two sets of remarks. First, three apes showed at least some understanding of the causal structure of the task, for they learned to behave in particular ways (cause) to obtain the reward (effect). Second, and far more important, it is again unclear what would count as evidence that apes invoke an a priori theoretical concept such as gravity. (Parenthetically, incredulity grows across the three expressions, "to invoke concepts," "to invoke theoretical concepts," and "to invoke a priori theoretical concepts.") Povinelli might argue that if the apes had varied their tool-inserting behavior with the orientation of the trap, then they would be showing understanding of the causal structure of the task and, more specifically, that the concept of gravity mediated their performance. Although the expectation that without specific training the apes would vary their behavior with the trap orientation seems extraordinary, for the sake of argument let us assume it was fulfilled. What further understanding of the animals' performance, the variables influencing it and their interaction, among other things, would be gained by appealing to the concept of gravity? Also, in this instance, would gravity mean the everyday sense of weight or the technical sense of the force of attraction between two masses? Going one step further, are chimpanzees supposed to represent not only the gravity of fruits and rocks but, in good Aristotelian fashion, also the levity of smoke and fire? It seems

⁶ In the trap-tube experiment it was not even necessary to attend to the trap; it was sufficient to choose the opening farthest away from the reward. Other studies cited by the author do suggest that chimpanzees learn to insert the tool into the opening that allows them to push the tool and move the reward *away* from the trap.

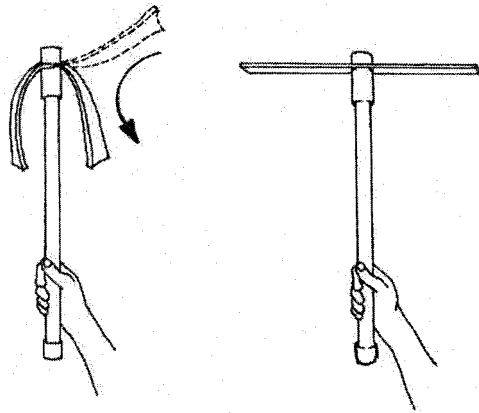


Fig. 3. Flimsy and rigid rake. From Povinelli, D. J. (2000). *Folk physics for apes: The chimpanzee's theory of how the world works*. Oxford, England: Oxford University Press. Reprinted with permission.

to us that had the apes varied their behavior with the trap's location we would be better off admitting our puzzlement and lack of understanding than creating the illusion of understanding by invoking gravity—or the apes' difficulties in “keeping too many causal principles (and too much background knowledge) in mind at the same time” (p. 131).

Flimsy-Tool Problem

In another interesting experiment, Povinelli asked whether chimpanzees possess “a conceptual understanding of the relation between the rigidity of a tool and its ability to move an object” (p. 163). To answer the question, the author presented the animals with two similar rakes, one with a rigid base that could be used to drag an out-of-reach apple, and another with a flimsy base that could not drag the apple (Figure 3). As before, the apes first played with both tools before they participated in a series of formal tests. Given two rakes, would the chimpanzees prefer, or eventually learn to prefer, the rake with the rigid base? More generally, would the apes' choices be more consistent with an understanding of the “folk physics of transfer of force” (p. 161), the high-level model, or with the learning “of another specific procedural rule” (p. 161), the low-level model? The distinction between the two models is further described as follows:

If, during the experiments . . . the subjects had only come to construct a procedural rule con-

cerning the importance of the base of the rake being in such-and-such a spatial relation relative to the reward, or even physically contacting the reward [several other experiments had used rakes], they would have no reason to appeal to a folk notion of transfer of force. On the other hand, if their ultimate success was framed in terms of an understanding that contact is necessary for the transfer of force, they might also be expected to appreciate that a highly flimsy object cannot effectively transfer force to an object of considerable mass. Thus . . . we asked our apes not about the possibility of physical contact between the tool and the reward, but about the relative effectiveness of such contact. (p. 164)

The experiment included two types of trials. On control trials, the chimpanzees were given a rake with a solid, rectangular base. This rake could be used to drag a piece of apple within reach. Because the apes had learned to use this tool in a previous study, they had no difficulties in successfully using it again. On test trials, the chimpanzees were presented with two rakes, placed side by side on a table: one rake with a solid base and another with a flimsy base. After the ape entered the room, the experimenter lifted one of the rakes and “repeatedly tapped on the base to demonstrate its rigidity In the case of the ineffective tool, the experimenter held up and shook the tool (causing the rubber strip to flop up and down), and repeatedly lifted the stripping and let it drop” (pp. 166–167). The tool was then returned to the table and half of an apple was set directly in front of it. Next, the experimenter lifted the other rake, “demonstrated its rigidity,” returned it to the table, and baited it with another piece of apple. After this demonstration, approximately 1-min long, the chimpanzee left the room, waited for 15 s, reentered the room, and made its choice. Each daily session included four control trials and one test trial, and a total of eight sessions were conducted.

The results showed that for 6 of the 7 apes choice proportions did not differ from chance. The exception was Jadine, who chose the solid-base rake on all test trials. The performance of this ape casts doubt on the hypothesis that the apes simply could not remember which tool was correct after reentering the room. Povinelli also remarks that the bases of the tools were visually dis-

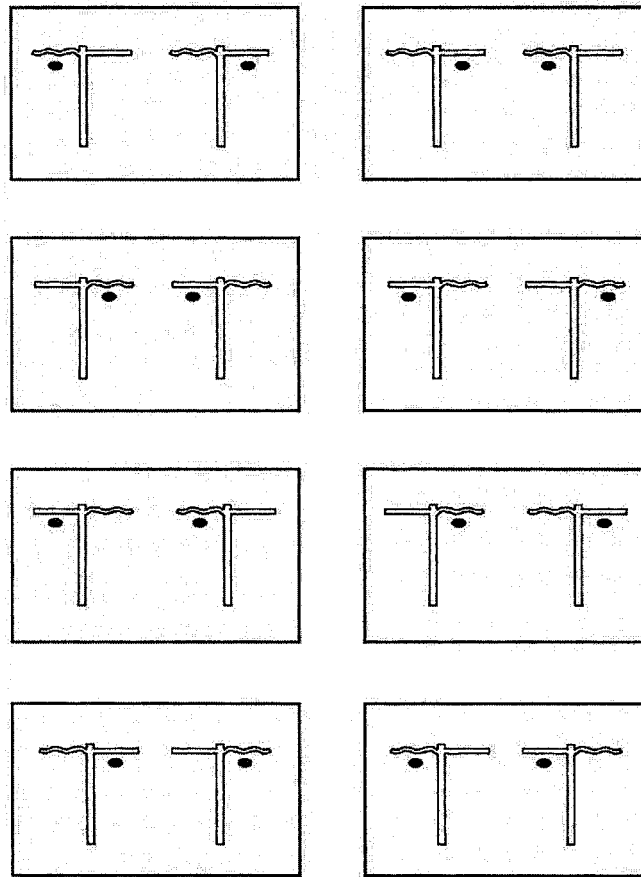


Fig. 4. The eight types of test trials used with Jadine. From Povinelli, D. J. (2000). *Folk physics for apes: The chimpanzee's theory of how the world works*. Oxford, England: Oxford University Press. Reprinted with permission.

tinct in that the flimsy rake was not completely straight. Returning to Jadine's performance, should it be concluded, then, that she understood the concept of rigidity? Povinelli remained skeptical, and in a moment that would have made William of Occam and Lloyd Morgan proud, Povinelli remembered that during a previous study his apes had been exposed to rubber snakes and, of all the apes, Jadine had been the most wary of these objects. Hence, she might have been simply avoiding what looked like a rubber snake.

To test this hypothesis, the author exposed Jadine to a new set of clever tests. Two identical rakes were built, each having one half of its base made of solid plywood and the other half made of a rubber strip. By systematically varying the relative orientation of the rakes and the location of the rewards, a total of eight configurations were obtained (Figure

4). If Jadine had been simply avoiding the flimsy tool due to stimulus generalization from rubber snakes, then she would behave randomly on the new trials. Alternatively, if she had understood the importance of the "rigidity/mass interaction" (p. 169), then she would consistently choose the rake with a reward in front of its solid half base. Jadine chose the correct tool on four test trials and the incorrect tool on the remaining four, the outcome most expected by chance responding. Povinelli concludes:

[T]he results . . . provide no evidence that our apes were recruiting conceptual knowledge of the connection between the rigidity of the rake tools and the mass of the apple which needed to be moved. Despite the extensive and specific experience that they received during the previous eight experiments [all involving the use of tools], our apes appeared obliv-

ious to the importance of a property of a tool that had obvious implications for retrieving an object—its rigidity. Even after allowing the animals to obtain direct experience with the nature of these tools in the context of free play, and further demonstrating the relevant properties of each tool immediately prior to allowing them to respond, our apes behaved exactly as if they did not understand how the significance of the differing properties of the bases would affect the movement of the apples The apes were remarkably insensitive to the differential feedback they received on each trial In contrast, . . . by 24 months of age, human infants will avoid using non-rigid implements in simple reaching tasks. (pp. 171–172)

Implicit in these remarks is a questionable conception of learning. According to Povinelli, the apes had ample opportunity to learn about the importance of tool rigidity during previous experiments, but, as far as we could ascertain, the apes were never taught to distinguish rigidity. In the absence of differential training, it is unclear what sort of learning process could yield sensitivity to rigidity. During the flimsy-tool experiment there was differential training, but the characteristics of the training were far from optimal to promote learning (e.g., few training trials, presence of retention interval). Even if training had persisted, the apes' behavior would probably not be influenced by the rigidity dimension because salient dimensions such as shape, which covaried with rigidity, might overshadow it. This process of overshadowing is potentially important for another reason; namely, it questions the experimenter's putative case of demonstrating the rigidity of the tools. To assume that by shaking the tools the experimenter demonstrated their rigidity to the apes is akin to assuming that by pointing to a square of red cardboard and saying "red" a parent will teach about the color red to his 2-year-old child. But how and why would the child learn about red as opposed to, say, square, cardboard, rough texture, or even the tip of the parent's finger? To be effective, ostensive definitions and demonstrations require a great deal of previous learning. Abstractions in particular require, at a minimum, that stimulus dimensions be uncorrelated.

MISUNDERSTANDING "UNDERSTANDING"

Having discussed some specific experiments in the preceding sections, we comment now on some broader conceptual aspects of Povinelli's approach and his ultimate goal of discovering what chimpanzees *appreciate*, *know*, and *understand* about tools and others' minds. Unfortunately, as we argue in this section, he misconstrued the concept of understanding and, in doing so, made it even more unlikely that the outcomes would favor the high-level model. Nevertheless, Povinelli used these negative outcomes to draw a set of conclusions and sketch alternatives about chimpanzees' reasoning abilities. Interestingly, as we will show in the last two sections of the review, his conclusions rejoin some old ideas from behavior analysis and his suggested alternatives have a decidedly behavioristic core, appearances notwithstanding.

Let us begin by recalling that in the experiments about seeing and intention, the author wanted to explore whether chimpanzees understand that others see or that actions can be categorized as intentional or unintentional. In other studies, Povinelli attempted to determine whether chimpanzees possess "a genuine understanding of attention" (p. 43) or whether they were simply "behaving *as if* they understood something about. . . a mental event" (p. 39). According to Povinelli, a strong demonstration of understanding requires that a chimpanzee engage in behavior that is appropriate to a particular circumstance without having been explicitly trained to engage in that behavior in that circumstance. In his experiments, conditioning was restricted to a minimum to keep "the *important trials* [*italics added*] [the test trials] as novel as possible" (pp. 34–35). "We wanted to minimize their [the apes'] rate of learning—after all, we were interested in what they understood ahead of time, not what they could learn through trial-and-error" (p. 35).

Although there is nothing wrong with Povinelli's first criterion of chimpanzees' understanding, his second criterion expresses an empirically naive and a conceptually untenable view of understanding. Empirically, understanding often relies on concept learning, which itself relies on the more fundamental phenomena of stimulus generalization. The

problem is that generalization is often unlikely, even in humans, without explicit generalization training (Martin & Pear, 1999; Stokes & Baer, 1977). Ask any kindergarten teacher and you will be told about explicit generalization training that is necessary for children to learn colors and shapes, or that $1 + 2$ is the same as $2 + 1$. Numerous stimulus exemplars are used daily across a variety of settings to promote generalization: worksheets with shapes and colors presented in different orders and sizes; colors and shapes presented individually and in groups; songs, poems, and games to learn about colors and shapes; guided arithmetic practice done on paper while thinking silently and at the chalkboard while thinking aloud. Understanding requires concept learning, concept learning requires generalization, and generalization often requires generalization training. Without this training, generalization and thus understanding is unlikely.

To be accurate, Povinelli does not say that understanding cannot result from learning; he simply undervalues understanding that arises from explicit operant conditioning while he overvalues spontaneous, naturally occurring understanding. This distinction is conceptually untenable. A kindergarten teacher teaches children to identify shapes and colors: Is the understanding that results from this teaching explicitly and programmatically trained or naturally occurring and spontaneous? Some parents purchase puzzles, books, and games that help them teach their children to correctly identify colors, shapes, and letters. Other parents may simply say in response to a child's labeling of a shape or color, "That's right," or "No, that's red." Is the child's eventual understanding naturally occurring or explicitly conditioned? Behavior has consequences, whether natural (in the sense of unplanned) or programmed. For organisms sensitive to those consequences, operant conditioning is as natural and spontaneous as breathing; unless, that is, one can show that spontaneous and explicitly trained forms of understanding are achieved through distinctly different behavioral or neural processes. Commenting on other concept learning research, Povinelli remarks:

Typically, animals are trained on hundreds or even thousands of trials, and are then tested

for their abstraction abilities using somewhat novel stimuli, or novel arrangements of familiar stimuli. In many cases, the species in question will display an impressive ability to generalize to the new stimuli, an ability which is interpreted as meaning that they have formed the relevant concept. However, even for those demonstrations that have been interpreted as the most persuasive evidence of conceptual abstraction (for example, the ability to use "abstract" same-different relations), alternative accounts that focus on the interface between the perceptual dimensions of the stimuli and the attentional activities of the animal can easily be generated. (p. 311)

Unfortunately, no alternative accounts were provided. Hence, lacking empirical evidence or a persuasive argument, we find no reason, and Povinelli advances none, to ascribe more importance to spontaneous understanding than to understanding that results from programmatic training. This is an instance in which the adjective *spontaneous* does double harm: it hides our ignorance about causes and perversely casts our ignorance as virtue.

Another consequence of inflating the value of spontaneous understanding is a lingering, uncomfortable tension in Povinelli's approach to research. On the one hand, he clearly saw the necessity to train the chimpanzees, to some extent, to attend to the critical features of the tasks; he did not simply familiarize the apes with the experimental setting and then test them. The reason for this course of action is obvious: without some training, the chances the animals would generalize appropriately would be low or nil. On the other hand, Povinelli seems to believe that explicit training precludes, by definition, a study of folk physics or folk psychology: "Determined to demonstrate similarity [between humans and apes], many researchers train on, without ever seriously considering the possibility that the very extent of the efforts required to produce human-like behaviors in their animals undermines the very claims they wish to make in the first place" (p. 338). In other words, explicit training prevents us from examining the chimpanzees' naturally occurring and spontaneous understanding.

This tension may be expressed in the form of a classical avoidance-avoidance conflict:

the cost of explicitly training understanding is that you are not, by definition, examining folk psychology or physics. Damned if you do. But the cost of not programmatically training understanding is that there is a very low or zero probability of it occurring. Damned if you don't. The tension does not vanish by casually dismissing initial training as pretraining or warm-up effects, or by arbitrarily setting the amount of training above which understanding is no longer spontaneous. The tension does vanish by eliminating the conceptual distinction that created it in the first place. There is no scientific reason to be impressed by spontaneous understanding or to be unimpressed by understanding that results from explicit conditioning.⁷

Ironically, Povinelli's preference for spontaneous understanding over explicitly trained understanding is analogous to Köhler's preference for understanding due to insight over understanding due to trial-and-error learning. Commenting on the latter, Pavlov (1957), who had also studied apes in similar situations, remarked during one of his famous *Wednesday's meetings*:

[Psychologists] apparently, want their subject to remain unexplained. How strange, indeed! How they love the mysterious . . . (p. 554). His [Köhler's] sole fundamental, but peculiar proof is this. When the ape is given the task of taking hold of fruit suspended at a certain height, and when for the purpose of accomplishing it he needs definite instruments, for example, a stick and some boxes, all his unsuccessful efforts to get the fruit are not, according to Koehler, proof of intelligence. This is simply the method of trial and error. When the ape becomes tired, as a result of his unsuccessful efforts, he gives up and remains for some time in sitting posture. When he has rested he tries again and succeeds in accomplishing his task. According to Koehler the ape's intelligence is proved by the fact that he

sits for a period without doing anything. He literally says that, gentlemen. In his view the ape accomplishes some kind of intellectual work when it is sitting, and this proves its intelligence. How do you like it? It turns out that nothing but the silent inaction of the ape proves its intelligence!

And the fact that the ape uses a stick and places several boxes one on top of the other, this is not a manifestation of intelligence. When the ape acts, moves the boxes from one position to another, these are associations and not manifestations of intelligence; this is the method of trial and error. Koehler absolutely disregards these facts—all this is simply only association. But when the ape rests, when he is inactive, he performs certain intellectual work. Naturally, the only explanation one can offer for such reasoning is that Koehler is a confirmed animist, he simply cannot become reconciled to the fact that this soul can be grasped by hand, brought to the laboratory, and that the laws of its functioning can be ascertained on dogs. He does not want to admit this. (pp. 558–559)

A BEHAVIOR ANALYST IN DISGUISE?

Twenty-seven experiments on chimpanzees' folk physics and about a half-dozen on folk psychology converged to refute the high-level models. Repeatedly, seemingly straightforward principles of learning predicted and explained the data better than seemingly sophisticated anthropomorphic views. What, then, did Povinelli conclude from his massive data set? Surprisingly, perhaps, he reached a set of conclusions that, like Janus's face, can be read in two different ways. From the positive side, these conclusions are consistent with many ideas defended by behavior analysts, both old and new. By the end of the book, we recognize in Povinelli the interpreter, theoretician, and speculator, a bona fide behavior analyst disguised only by a thin cognitive mask. From the negative side, the author's conclusions illustrate the old adage that those who ignore history are bound to repeat it. On multiple occasions, *Folk Physics for Apes* seems déjà vu, presenting old conclusions on the basis of new data. Below we identify some conclusions that a behavior analyst might consider to be obvious.

Argument by Analogy

Povinelli and Giambrone's major purpose in reviewing the folk psychology experiments

⁷ In some experiments, when the apes did not understand what Povinelli considered the critical features of the task, attempts were made to condition the animals to learn them. In most cases, these attempts were only marginally successful: learning was ephemeral, or only some apes learned to respond correctly during the test. Whether these results represent failures to learn, inadequate training, or both, is unclear. Although Povinelli cautions the reader not to devalue what the apes actually accomplished in his experiments, we suspect that the apes could have learned much more had they been trained using methods common in concept learning.

in the longest chapter in the book (Chapter 2, *Escaping the argument by analogy*) was to conclude that “*similar behavior—even among closely related species—does not guarantee a comparable degree of psychological similarity*” (p. 2). According to the authors, the opposite belief, the so-called argument by analogy which states that similar behaviors imply similar processes, is “deeply flawed” (p. 9). Although the authors’ critique is engaging and clear, their arguments are old and unnecessarily forceful. It is unlikely that many contemporary behavior analysts, comparative psychologists, ethologists, or zoologists would endorse the argument by analogy. We wonder why Povinelli devoted such a large portion of his book to knocking down what seems to be such an obvious straw man.

Introspection

Near the end of the same chapter, Povinelli and Giambrone present their reinterpretation hypothesis, which claims, among other things, that the ability to reason about mental states evolved long after the ability to reason about behavior. On the basis of this hypothesis, they claim the following:

The reinterpretation proposal has deep, and somewhat disturbing implications for understanding both human and chimpanzee behavior. In the case of humans, for example, it suggests that we have a far less accurate view of the relation between our mental states and our behavior than we are inclined to think Indeed, we suspect that most of the ancient psychological mechanisms which drive our moment-to-moment behaviors do not intrude into our conscious experience, and therefore we are frequently left to misdiagnose the psychological causes of our behaviors.

Thus one of the central implications of the reinterpretation hypothesis is that introspection is poorly suited to reveal the exact causal structure between our conscious psychological states and our overt behaviors The reinterpretation hypothesis argues not simply that introspection is the wrong tool for recovering the correct causal relation between mental states and behavior, but that its inadequacy in this regard derives precisely from the fact that the ability to describe behaviors in mentalistic terms evolved well after those behaviors were already up and running and being generated by other psychological mechanisms unrelated to the representation of mental states. (p. 65)

But how is this different from Skinner’s (1974) and others’ (e.g., Dunlap, 1912) analyses of the nature and limits of introspection, or of people’s propensity to misattribute the causes of their behavior? How is this different from the very analyses and concerns that led to behaviorism (e.g., Skinner, 1963, 1974, 1977; Watson, 1913)? Historians of psychology remind us that even further back, in the early 1900s, psychologists in America were filled with “the suspicion that introspection was a fragile and unreliable tool, easily prejudiced by theoretical expectations” (Leahey, 2001, p. 95) and some prebehaviorism psychologists “were ready to discard it altogether” (Leahey, p. 96).

Povinelli and Giambrone also conclude that mentalistic idioms are merely “convenient (and useful) ad hoc descriptions of our behaviors—behaviors that both can and do occur without such descriptions” (p. 68). Again, how is this different from what Skinner (1957, 1974, 1990) and others (e.g., Watson, 1913) have said many times and years before?

Povinelli (Re)joins Pavlov

After escaping the argument by analogy and rejecting introspection, Povinelli summarizes his major global conclusion about chimpanzees’ folk physics in the last chapter of the book:

The results of our investigations have convinced us that, although chimpanzees possess an excellent ability to reason explicitly about relations between objects and events that can be perceived, they appear to know little (if anything) about phenomena that are, in principle, unobservable [W]e contend that their central reasoning systems do not reason about things which have the status of being “hypothetical”. In our view, this is because the chimpanzee does not form such concepts to begin with. (p. 298)

Later, he concludes:

We must be careful to emphasize that we are not claiming that chimpanzees (or other animals) fail to form concepts about the world. On the contrary, there is very good evidence that many nonhuman species form a variety of “concepts” based on perceptual generalization gradients Rather, we suggest that the range of concepts formed by the chimpanzee does not include concepts about entities or

processes that have no perceptually-based exemplars. On our view, chimpanzees detect the regularities that exist between events, and learn to act on the basis of them, but they do not appeal to unobservable phenomena (force, gravity, etc.) to account for (or assist in their reasoning about) such regular associations of events. (p. 298)

These, too, are not novel conclusions. Pavlov had already anticipated them on the basis of his experiments with apes. In one task, for example, Pavlov's chimpanzees Raphael and Rosa faced the problem of getting a banana that was suspended from the ceiling of a cage by stacking boxes of different sizes in the right order. In another task, the apes could get a reward by inserting a rod into the opening of a box. The shape of the opening and the cross-section of the rod varied (e.g., round, quadrangular, triangular) such that only one rod from the bundle fit into the opening. In yet another task, Raphael had to use water to extinguish a series of candles that encircled a reward.

To make sense of his observations, Pavlov invoked external and internal inhibition, formation of elementary associations and their chaining, synthesis and analysis, generalization and discrimination, instincts and unconditional reflexes, imitation, and several other phenomena. Although we cannot do justice to the richness of his intuitions and interpretations here (but see Pavlov, 1957), the following summarizes his key points:

The ape's *thinking is clearly observed in his actions* [italics added]. And this is the proof of his intelligence. It shows that there is nothing in intelligence but correct or erroneous associations, proper or distorted combinations of associations. Koehler, however, maintains that it is not a matter of association. Meanwhile the entire intelligence consists precisely of associations. What distinguishes it from the development of a child, or from our inventions? For the ape the problem consists in getting the fruit without the help of a stick, and he does this before your eyes by the method of trial and error, i.e., by means of associations. It is absolutely clear. In what way does this differ from our scientific discoveries? It is exactly the same thing. Evidently, this is elementary intelligence, differing from ours only by the poverty of associations. *The ape has associations which relate to the interaction of mechanical objects in nature* [italics added] Thinking once

more about the reason for the ape's success compared with other animals, and why he is closer to man, we would say that it is precisely because he has hands, actually even four hands, i.e., more than we have. Because of this, *the ape can enter into very complex relations with the surrounding objects* [italics added]. And that is why a multitude of associations are formed in him, associations which do not exist in other animals. Since these motor associations must have their material substratum in the nervous system, in the brain, the cerebral hemispheres of the apes are more developed than those of other animals, this development being due to the diversity of their motor functions. We, humans, in addition to the diverse movements of our hands, possess a complex of *speech movements* [italics added]. As is known, the ape is less capable of imitating speech than many other animals That is how I see the matter. (Pavlov, 1957, pp. 561–562)

SKETCHING A BEHAVIOR-ANALYTIC APPROACH TO THE STUDY OF TOOL USE

Having rejected the high-level models, Povinelli naturally tries to sketch alternative accounts of his findings. In these sketches, two classes of variables become somewhat more prominent; the organism's experience or past history, and language or verbal behavior. After comparing the experimental results with chimpanzees and humans, Povinelli acknowledges the importance of experience in the construction of humans' folk psychology:

Prior to visiting our center, these children have had numerous semi-structured experiences of "seeing" and "not seeing" (in the context of playing with their parents and peers). Thus, long before participating in our tests, the children have been confronted with games, and even "real" social experiences in which they have to cope with the distinction between seeing and not seeing. Although their reactions to these situations were not linked to receiving or not receiving a sticker, surely the range of their responses were linked to a range of differential responses from parents and peers Given the amount of experience that children have with explicitly created instances of "seeing" and "not seeing" by their parents and siblings, how do we know that it is not precisely this experience that allows the child to create the idea of visual attention in the first place? (pp. 40–41)

The causality of interactions with the physical and social surroundings is also extended to chimpanzees. By the end of the book, Povinelli advances a strongly behavioristic⁸ hypothesis:

If . . . chimpanzees act upon objects in the world, detect specific regularities, and use them as default assumptions about how the world is likely to behave, then the kinds of effects we have reported in this chapter can rest quite comfortably alongside the sorts of actions chimpanzees perform all the time. (pp. 252–253)

Later he adds that:

. . . there are many ways in which species such as chimpanzees may develop tool-using abilities other than by explicitly appreciating the kinds of unobservable causal variable that are of such interest to even very young children. (p. 301)

Unlike humans, the chimpanzee's reasoning about both physical objects and social beings appears restricted to concepts, ideas, and procedures that are linked to the world of tangible things. In both the social and the physical case, the chimpanzee learns about the observable properties of these entities, and the kinds of behavior that these entities typically exhibit. The chimpanzee even takes the impressive leap of generalizing the new instances. But in neither case does the chimpanzee appear to generate additional concepts, related to perceptually non-obvious phenomena, concepts which could provide a unified account of why such regularities exist in the first place. No, this appears to be a specialization of the human species . . . [This specialization] may have been favored in human evolution because it allowed for a new degree of flexibility in both the social and physical realms. (pp. 338–339)

What is presumed true of chimpanzees is also presumed true of young infants, for they, too, "may be constructing knowledge about the *behavior* of objects in the world, not concepts like force, gravity, or mass." (p. 323)

This emphasis on interaction between the

organism and its surroundings—actually, on the causality of interaction—is a central theme of behavior analysis (and also of Piaget's developmental theory). Children explore objects and in the process discover their general properties and how they might relate to one another. A stick may be rolled over the floor or placed vertically; a piece of cloth may be crumpled or folded; a rake may be grasped by the handle or the prongs. Moreover, each of these objects may enter into multiple relations with other objects: a stick may be pushed, pulled, or inserted; a piece of cloth may wrap, retrieve, or support. The meaning of an object becomes more than its perceptual aspects because it is enriched by the actions it affords and, mediated by these actions, the relations it can entertain with other objects. Each child discovers this web of possible actions and relations by direct contact, observation learning, and instruction.

Stimulus generalization and discrimination, action differentiation, and action coordination are part of what any tool user learns. The following examples illustrate everyday circumstances in which this learning may take place in the case of young children:

1. Sticks with different properties (e.g., length and weight) require different ways of handling; some may require the use of both hands. Similarly, different intended trajectories require different ways of handling a stick.
2. Initially, a child may indiscriminately use a simple stick and a stick with a hook at one end. But when the child fails to retrieve a toy from a box using the simple stick and succeeds using the stick with the hook, the child begins to learn about hooks; when and how to use them. These contingencies of reinforcement and the interactions they promote increase the perceptual salience of parts of objects and isolate them functionally.
3. To achieve some goals, more than one action may be needed. A child might need to sweep a stick laterally to hit a toy and move it to the left, and then push the stick forward to hit the toy and insert it inside a box. Learning a plan of action or strategy requires learning to coordinate a set of actions in a given sit-

⁸ In a gratuitous remark, the author wrote: "This is not to say that the apes were acting like unconscious Skinnerian learning machines, but rather that the information that they processed, stored, and came to act upon may have been about perceptual task features, not causal structure" (p. 204). We note that it is Povinelli, not Skinner, who invokes an unconscious machine, the computer, to explain behavior.

uation, to emit each action in its appropriate stimulus context and to execute it to bring forth its appropriate consequence (e.g., a response chain). The child must learn the action class called for in a given stimulus circumstance and the parameters of the appropriate response. Furthermore, given enough training, the person may eventually anticipate action outcomes.

An approach to tool use based on the key idea that causality is in the interaction will cause us to pay attention to the behavior of the organism in a problem-solving situation: to how, where, and when those responses occur for the first time; combine with other actions; change their parameters; disappear; and the like. This is the approach followed by Pavlov in his ape studies:

[I]t is the processes disregarded by Koehler that are of greatest importance. I grasped and realized this while observing the behaviour of the ape. And I say that all this activity of the ape in trying now one, now another way of solving the task, is the intelligence, the reasoning in action, which you can see with your own eyes. This is a series of associations; some of them have been acquired in the past, others are formed before your eyes, and are either combined, united into a positive whole, or, on the contrary, are gradually inhibited and lead to failure. One can clearly observe the manifestation of some of the associations formed earlier in the ape, in the course of his life in the jungle, in his native surroundings. (Pavlov, 1957, pp. 558–559)

The study of interaction may be insufficient to explain complex instances of tool use, or species differences in tool use (cf., chimpanzees vs. orangutans), but it is the most obvious, accessible, and empirically testable candidate. Consider the following story. Sam, a 3-year-old boy, and Paulina, his 6-year-old sister, were asked on separate occasions whether it would be easier to run across the lawn while pulling a rake with its prongs facing down (against the grass) or up (toward the sky). Despite their differences in age, both children indicated that it would be easier with the prongs facing down against the grass, “because that’s the way a rake works,” said Paulina. However, after she tried to run while pulling the rake across the lawn, she changed her answer to, “It’s better to run

with the teeth facing up because otherwise they snag the grass.” To understand Paulina’s initial response we need to look into her experience with how people *use* rakes and how she has *used* them before. That she changed her answer after trying to run with the rake in different orientations reflects the combined effects of her past and current experiences with the tool, her ability to describe her actions, and her ability to understand the problem she was asked to solve.

As the preceding example shows, for humans, verbal behavior greatly enlarges the domain of interaction. And yet, the role of language in the formation of concepts, particularly those related to folk psychology and physics, is conspicuously absent from most of *Folk Physics for Apes*. In fact the term “language” probably does not appear more than 10 times in the entire book, and the index only refers the reader to seven pages where the subject matter is addressed briefly. We do not know how to interpret this glaring omission because, on the one hand, Povinelli seems to be aware of the importance of language, but on the other hand he rarely tackles the issue directly and explores its implications in depth. In one of the few occasions in which language is discussed, however, Povinelli writes that “representations of hypothetical entities [e.g., force, gravity] may be impossible without human-like language, or perhaps more directly, language may have created such representations” (pp. 298–299). In the same vein, having first entertained the hypothesis that chimpanzees interpret and explain the world,⁹ Povinelli questions this hypothesis at the end of the book: “Thus, we question whether the great apes ‘explain’ or ‘interpret’ the world in any real sense whatsoever” (p. 339). A behavior analyst would probably agree with the first conclusion, but at the same time remark that the second conclusion is not even a meaningful statement, for nonverbal creatures do not *explain* or fail to explain the world, or use or fail to use concepts to *interpret* regularities between events. The statement, “apes do not explain or interpret the world,” and its negation, “apes

⁹ Here is an example: “How does the ape *come to explain* [italics added] why in some cases their actions on an intermediate object (a tool) yield covaried movements in a goal object, but in other cases do not?” (p. 252)

explain or interpret the world," cannot be true or false because they are meaningless.

Although Povinelli suggests that experience and language may be necessary to the formation of a folk physics and a folk psychology in humans, it seems agonizing for him to accept this possibility. He addresses this issue in the subsection *Isn't it all just a matter of experience?* in the section entitled *Skeptical concerns and replies* in the book's last chapter, and surmises that "experience alone will not lead chimpanzees to construct concepts for which evolution did not adequately prepare them" (p. 326). To test his surmise, he suggests a carefully controlled cross-fostering study in which infant chimpanzees are reared with humans rather than members of their own species to see if chimpanzees that received the same kind of experiences as human infants routinely do would arrive at the same beliefs. If the experiment were conducted properly, "it would stand as one of the most important achievements in the history of the cognitive sciences" (p. 326).

As captivating as such a study is at first sight, we believe it would tell us little about the formation of folk psychology and physics. Methodologically, a cross-fostering experiment assumes that it is somehow possible for a human "parent" to raise a chimpanzee like a human child, but this assumption overlooks or downplays the fact that a chimpanzee "child" will also affect a human's parenting. Developmental psychologists and parents are keenly aware that boys and girls help to transform men and women into fathers and mothers. Povinelli's own refutation of the argument by analogy also casts doubt on the feasibility of keeping the experiences of a human infant and a human-reared chimpanzee "the same." Conceptually, experiments to test whether nonverbal creatures can learn concepts such as gravity or force reminds us of Wittgenstein's (1958b) remark: "We say a dog is afraid his master will beat him; but not, he is afraid his master will beat him tomorrow. Why not?" (p. 166e). Because a concept like "tomorrow" (or "gravity" or "force") is an integral part of a language game, and it derives its meaning from its uses in the game. It simply cannot be learned effectively without a language. Finally, at a fraction of what such a study would cost and at the gain of potentially greater profits, one could describe

in finer detail and classify the interactions of the chimpanzees with the tools and also try to teach them one of the folk physics' concepts, and then examine through transfer tests what they had learned.

In summary, Povinelli suggests that language is necessary to learn concepts that refer to inferred and highly abstract properties (e.g., gravity, force, tomorrow). Deprived of these concepts, chimpanzees and infants reason exclusively on the basis of concepts with directly observable referents. After many experiments, countless tests of high- and low-level models, some conceptual infelicities, and a great many null findings, the book's final precipitate is surprisingly behavioristic at its core.

CONCLUSIONS

From the perspective of a psychologist of learning, *Folk Physics For Apes* is a fascinating piece of detective work on stimulus control and concept formation, on how tool use is influenced by elementary stimulus features such distance and contact, and by other, not-so-elementary, features such as tool orientation, perceptual containment, relative position, or the graspable end of tools. We learn how the actions of chimpanzees are differentiated, connected to specific contexts, generalized along different dimensions, coordinated with other actions, and varied upon, all within novel and clever experimental arrangements. Additional positive features of the book include a relatively extensive discussion of Köhler's work—it is not what one reads in introductory textbooks!—and a set of interesting speculations relating self-recognition in the mirror to kinesthetic cues. In many ways, the reasons that motivated the experiments, the implausible and often incoherent high-level models, are the least important reasons to read the book.

What we often take for granted is not always what we best understand. Given the extraordinary claims made these days about animal minds, their consciousness and feelings, their arithmetical and statistical abilities, and the like, it is also important to read that:

... under the far more controlled circumstances [i.e., in the laboratory, not the wild] we have regularly observed many an apparent brilliant instance of tool use, only to discover

time and time again (through the proper control tests) that our common-sense interpretation of these behaviors were mistaken. Perhaps the most fundamental lesson that we have learned from our research is that our naïve interpretations are typically blind to psychological processes that differ substantially from our own. (pp. 327–328)

On a more human level, the book exposes a side of scientists not often seen in contemporary writings. In what we found to be an attractive characteristic of the book, Povinelli readily shares his thoughts and feelings about the outcomes of his experiments, as well as about other investigators and theorists (not unlike Pavlov's comments about Köhler quoted above). We enjoyed seeing a scientist wear his heart on his sleeve and expressing his passion for and against a variety of experiments and theories.

I diligently recorded and reported the hundreds of trials it took the chimpanzees to learn what, in truth, seemed like a rather basic problem—a problem that should have been well within the abilities of the chimpanzee genius that I had learned about in my youth. But rather than confronting the basic findings directly, I found excuses for them. That was easy. After all, in the sometimes cult-like atmosphere surrounding studies of chimpanzees, apologies came ready-made It was only later . . . that, like a drunken man slowly sobering, I began to understand that many of my most cherished beliefs about chimpanzees were based on faith, not evidence. (p. x)

It is these expressions that allow the reader to witness the metamorphosis of Povinelli's thoughts. The author first believes in high-level models, then confronts the experimental findings and is almost incredulous about them, and then seems to reject the high-level models and all they tacitly assume. In the end, however, despite the overwhelming success of the low-level models in predicting the results, Povinelli chooses not to embrace

them clearly but to cover the results under a speculative evolutionary scenario. Graduate students and professionals alike will find much to fascinate, entertain, and debate in this book.

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